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SYSTEM ACQUISITION STRATEGIES

Robert Perry, Giles K. Smith, Alvin J. Harman and Susan Henrichsen

A Report prepared for

UNITED STATES AIR FORCE PROJECT RAND

AND

ADVANCED RESEARCH PROJECTS AGENCY

Rand

SANTA MONICA, CA 90406

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PREFACE

Virtually since its beginning as a research center, Rand has been examining the process of developing and buying major weapon systems. Studies have extended over a range of topics from consideration of the effectiveness of different types of contracts to an evaluation of procedures appropriate to the acquisition of one small set of components. The acquisition of major weapon systems has become of particular concern lately because new weapons reaching the operational inventory often cost substantially more than was initially proposed. That is not a novel phenomenon, of course, although it is receiving renewed attention at present.

In the spring of 1969, the Deputy Chief of Staff, Research and Development, for the United States Air Force, and the Director of Defense Research and Engineering (DDR&E) approached Rand almost concurrently with a set of questions that arose from increasing concern about this cost growth syndrome. The Air Force was interested in the possibility of improving the accuracy of the cost estimating process that underlay early proposals. DDR&E wanted to know if it was possible to distinguish between the program management effectiveness of the 1950s and the 1960s and to identify those elements of acquisition policy that were responsible for any important differences. Within Rand, a set of intensive studies of the weapon system acquisition process grew out of these inquiries and from the base of earlier research. This report is a review and summary of some of the research and findings of the Rand system acquisition study in 1969 and 1970 and a statement of some policy implications. Much of the detailed work performed by various members of this group has been separately reported to both DDR&E and the Air Force. Three reports had been published by 1971,* and several others are in preparation. However, owing to the rapid pace of the work and

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*R. L. Perry, D. DiSalvo, G. R. Hall, A. J. Harman, G. S. Levenson, G. K. Smith and J. P. Stucker, *System Acquisition Experience*, RM-6072-PR, November 1969; Alvin J. Harman, assisted by Susan Henrichsen, *A Methodology for Cost Factor Comparison and Prediction*, RM-6269-ARPA, August 1970; Arthur J. Alexander, *R&D in Soviet Aviation*, R-589-PR, November 1970.

the relatively high interest of both the Air Force and DDR&E in being advised even of preliminary and interim findings, much of the research has been reported in briefings. This report is largely concerned with work not previously described in written form. Much of the research reviewed here will be covered in greater detail in reports now being prepared.

This report should be of particular interest to the various DoD, USAF, industry, and research-institute groups concerned with possible changes in present system acquisition procedures, processes, and institutions.

SUMMARY

Notwithstanding determined efforts during the 1960s to improve the outcome of major system acquisition programs by altering contractual approaches and by introducing a variety of management reforms, typical programs continued to exhibit an average cost growth of about 40 percent (corrected for quantity changes and inflation), a schedule slip of about 15 percent, and final system performance that was likely to deviate by 30 or 40 percent from the original specification. Such findings have been reported in earlier studies. During the present study, an examination of the basic factors contributing to cost growth suggested two avenues for improvement:

1. The cost estimation process could be improved so that cost predictions made early in a program would more nearly correspond to the cost actually experienced.
2. More fundamental improvements seem likely to be achieved through some basic changes in the acquisition process.

A potentially important step in cost estimation improvement would be to incorporate explicitly in the estimating relationship a measure of the technical advance sought in the program, because an analysis of past programs suggests a strong correlation between the degree of advance sought and the subsequent cost growth. However, cost estimating improvements of this type serve only to reduce the unpleasant surprises caused by unanticipated cost growth and do little to improve the acquisition process itself.

Observations of the outcomes of some recent European weapon system programs and some U.S. weapon system programs that were conducted outside the normal DoD acquisition channels suggest that two more basic changes are worth considering. Both represent an attempt to cope with the substantial uncertainties and inevitable revisions that occur in military hardware programs after they have begun development. The first change is to separate the development phase from the subsequent production phase, both sequentially and contractually. Such an adjustment should appreciably reduce the incidence of very expensive

production line changes that are often caused by technical problems discovered late in the development program. The available evidence indicates that a sequential, incremental acquisition strategy would not appreciably increase the time needed to progress from development start to the first true operational capability. The second suggested change is to conduct the initial portion of the development phase in a highly austere manner, concentrating first on demonstrating system performance while deferring the more expensive tasks of detailed production design and demonstration of reliability. This early emphasis on performance demonstration should reveal most of the technical problems at a fraction of the cost characteristic of current programs. Furthermore, initial austerity should in many instances permit multiple source development, if that is otherwise desirable, so that a truly competitive stance could be maintained in contract negotiations much further into a program than is now the case.

Finally, it seems clear that any system acquisition policy should be flexible. The changes outlined above should be applied to individual programs as individually appropriate. However, the evidence suggests that the normal mode of acquiring weapon systems during the 1970s probably should be based on an incremental acquisition strategy, with the exceptions being determined by such special considerations as might occur.

ACKNOWLEDGMENTS

The research and findings reported here are not in any sense the products of the authors alone. This summary captures the substance of work performed by a large number of Rand researchers, including James Stucker, Dominic DiSalvo, and William D. Putnam, who have been concerned with the schedule and performance implications of the data; Arthur Alexander, whose study of the development of aircraft in the Soviet Union has been published separately; Vangie Johnson, whose studies of the development of the Agena-D vehicle provided one of the principal examples supporting the proposals Rand has made; Fred Timson, who has dealt with cost estimation methods and with the prototype process; Gil Levenson and Robert Petkun, who have been concerned respectively with cost analysis and management control processes; and George Hall, who has been a long-term contributor to this research area and who, with Jack W. Ellis, provided valuable suggestions and advice on an early draft of this report. As the work proceeded, a large number of individuals within DDR&E and the Air Force provided assistance in obtaining and analyzing data. It is not feasible to list them all, but some must be mentioned: Vice Admiral Vincent dePoix and Mr. Harold Wakefield of DDR&E; Lt. General Otto Glasser, Air Force Deputy Chief of Staff for Research and Development; and Major General F. M. Rodgers, who at the time the work was conducted was Director of Development Planning in the Air Force Systems Command. Finally, Dr. John Foster, Director of Defense Research and Engineering, has been a principal advocate of circulating and encouraging widespread discussion of the initial findings. For any failings and shortcomings of this report, and for any deficiencies in reporting the findings of other researchers, the authors are solely responsible.

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I. THE WEAPONS ACQUISITION SCENE

The original objective of the Rand system acquisition study was to determine the true extent of program growth* in major Department of Defense weapon systems during recent years and to establish, if possible, the causes and indicators of differences between predicted and actual outcomes of major programs. Subsequently, an effort was made to propose improved procedures for estimating the outcomes of programs; although this effort encompassed the refinement of classic cost estimating procedures, it was not limited to that approach. Finally, as the work proceeded it became apparent that even on the strength of preliminary findings it was possible to propose changes in present acquisition procedures that seemed likely to improve the control of the system acquisition process.

The initial step in the study was to develop a detailed and reliable set of historical data on recent development programs so that subsequent analysis would be based on documentable facts rather than on casual or subjective observations. The request from DDR&E to compare programs of the 1960s decade with those of the 1950s, in terms of how well program cost, schedule, and performance outcomes compared with predictions, made it possible to secure data from all three services. With DDR&E assistance, Rand researchers obtained basic information on 21 major weapon systems developed during the 1960s, representing acquisitions of each of the three services. The systems originally proposed for inclusion were selected by DDR&E with the advice of the services. Owing to a number of difficulties arising in the nature of the data requirements and the development status of several of the systems in the original list, it was not possible to accumulate complete and consistent data for all of the systems. In the course of the study, however, data became available for several systems not in the original sample. Useful data covering a total of 24 major systems or principal

*Throughout this report the term "program growth" will be used to represent the differences between the actual cost, schedule, and performance outcomes of a program and the predictions made at the beginning of weapon system development.

subsystems eventually constituted the data base for the 1960s (see Table 1). As can be seen from the listing, the six Army systems include missiles, helicopters, and a tank; the eight Navy systems include aircraft, torpedoes, sonars, and a sonobuoy; and the ten Air Force programs include various kinds of aircraft and missile systems and major subsystems. Although this sample is limited, it is representative of the types of major systems acquired by the Department of Defense in the decade of the 1960s. The total dollar value of these programs is in excess of $23 billion.* Of this amount, about 80 percent is accounted for by the Air Force systems; almost 50 percent of the total is attributable to the cost of two very large Air Force programs -- the C-5A and the F-111.

During analysis of the data, the initial emphasis was on measuring the amount of cost growth experienced in each program. Cost growth was measured in terms of the differences between the actual costs and estimates made at the beginning of system development, with results expressed in terms of a "cost factor" -- the ratio of the actual cost to the estimated cost.

Since cost factors were used as the basis for describing and comparing the cost experiences of acquisition programs, two essential points concerning their limitations must be considered. First, such ratios capture a number of influences on program outcome but do not provide information sufficient to distinguish among these influences. And second, cost factors alone are inadequate for making cost comparisons since different programs may be more or less difficult to estimate accurately or carry out successfully.

A cost factor captures several influences on real or apparent program outcomes. Optimism in cost estimating -- or, as it is frequently categorized, the optimism inherent in the advocacy process that precedes the approval of new system developments -- is one

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*Since cost data were not available for all systems, it is difficult to estimate the total dollar coverage.

Table 1

SYSTEM COVERAGE

ARMY	NAVY	AIR FORCE
Cost, Schedule, and Performance		
Pershing I	OV-10A	F-111
Pershing IA	DIFAR	C-5A
OH-6A (Hughes)		C-141
		Titan III-C
		Minuteman II Airborne Command Post
		Minuteman II Guidance and Control
Cost and Schedule Only		
	A-7E[a]	A-7D[a]
	SQS-26AX[a]	
	SQS-26CX	
Schedule and Performance Only		
Sheridan	MK-48 Mod 0	XC-142
Cheyenne	A-7A	
Lance		
Schedule Only		
	MK-48 Mod 1	Sprint
Performance Only		
		SRAM

[a]Only actual schedule data were available for these systems. Without appropriate predicted milestone dates, no schedule factors could be generated for inclusion in Figure 1.

obvious contributor to the difference between actual and estimated costs.* Imprecision in the cost estimating process itself is also reflected in the cost factor. In addition to these influences, the cost factor also captures cost growth that occurs as a result of scope change and technological uncertainty. The term "scope change" is used here to identify changes in program goals or specifications after the start of development. Scope change can include any fundamental change in program objectives except changes in the quantities of systems ordered.** Technological uncertainty -- which may be, in part, another aspect of optimism -- arises in the expectation that technical problems can be resolved without causing a given program to consume more than the total of resources programmed at the time system development began. Finally, a cost factor also captures inefficiences in the development process, whether they arise from erroneous development strategies, inappropriate contract arrangements, or any of a multitude of conceivable management inefficiencies on the part of either the customer or the supplier. Cost factors alone provide insufficient information to distinguish among these contributors.

Therefore, cost factors alone should not be used to compare cost experience of different programs. A comparison might be made, for

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*If cost factors are to be used to measure cost growth, they should be calculated from estimates that have not been inflated or understated in response to such circumstances as funding pressures. If the estimate for one system is adjusted because of anticipated ease or difficulty in obtaining funding while the estimate for another system is not, comparison of the cost factors derived from these estimates would be meaningless. Among the systems in the sample used in the analysis reported here, there is no reason to believe that there were important differences in the pressures to obtain funding. It is thus reasonable to assume that cost factors can be used as a basis for making cost comparisons among these systems.

**Whenever quantity changes occurred during a program, an adjustment was made to reconcile the estimated cost with the actual quantity produced. Adjusting the estimated cost to the quantity covered by actual program cost is obviously preferable to calculating a cost factor based on different quantities. The basic data also were adjusted to compensate for inflation that occurred during the decade. The means of making these adjustments have been reported in the Appendix of Harman and Henrichsen, *A Methodology for Cost Factor Comparison and Prediction*, RM-6269-ARPA, August 1970.

example, between two programs having cost factors of 1.2 and 1.6, which would describe, respectively, cost growths of 20 and 60 percent. The cost factor of 1.2 could represent the outcome of a rather simple program utilizing off-the-shelf components, while the second program might involve the development of a very advanced weapon system built around several major subsystems about which there was great technical uncertainty at the time program development began. In such a case, the cost factor of 1.2 might suggest a badly structured, inefficiently managed program, while the apparent 60 percent cost increase of the other program might in retrospect be considered money well spent. Without more information on the programs in the sample than their respective cost factors, it would not be reasonable to say very much about individual program effectiveness, nor would it be possible to make adequate cost comparisons among systems. Any attempt to describe the contributors to cost growth or to make adequate cost comparisons must take into account the influence on cost of other program attributes, including especially program duration and technical difficulty.

The cost factors for programs of the 1960s are displayed in Table 2. The original expectation that the estimates contained in the formal Technical Development Plan might be used as a baseline for 1960s program outcome analysis was frustrated when, early in the research phase, it was discovered that Technical Development Plans were not available for a number of systems in the sample. A variety of other sources were used instead.* Nonetheless, every effort was made to obtain, as the earliest benchmark, an estimate valid at the time the DoD approved the start of weapon system development. The underlying assumption was that at such a point in time, the cost, schedule, and system performance requirements of the program had been extensively reviewed by the responsible developers, by the using service, and by approval authorities in the Department of Defense.

In general, program cost growth for the 1960s (corrected for inflation) has averaged about 40 percent. The cost growth in the

*The sources of the data actually used in the research have been reported in Harman and Henrichsen, RM-6269-ARPA, Appendix.

Table 2

DEFLATED COST FACTORS -- THE 1960s SYSTEMS
(total program costs as of mid-FY1970)

Air Force		Navy		Army	
System	Factor	System	Factor	System	Factor
F-111	2.07	A-7E	1.40	OH-6A (Hughes)	1.09
	1.95				
	2.02	OV-10A	1.10	Pershing I	1.12
	1.41				1.01
		SQS-26AX	2.34		
C-5A	1.36			Pershing IA	1.07
	1.38	SQS-26CX	1.55		1.03
	1.09				
		DIFAR	2.05		
C-141	1.16		1.04		
	1.41				
A-7D	1.23				
Minuteman II Airborne Command Post	1.12				
	1.28				
Minuteman II Guidance & Control	1.60				
Titan III-C	1.06				

The listing of multiple cost factors for individual programs indicates that different "initial estimates" were made at various times between program conception and program completion. The method used to reconcile such data is described in Harman and Henrichsen, RM-6269-ARPA.

development phases of programs is very difficult to establish but seems to average 50 to 100 percent.* The difficulty of establishing precise development cost factors arises very largely from the cost reporting system used by the services during most of the 1960s. If the development process is assumed to be that effort required to accumulate the information necessary to begin series production of an operationally acceptable system, then some portion of the money actually spent in the period after development formally ended (that is, work required to make the initially produced system satisfy real operational requirements) should be charged as part of development cost. In most cases, it proved either difficult or impossible to obtain an accurate accounting of the sums spent on development.

Schedule and performance factors were also considered in the analysis. Characteristically, there was an average 15 percent slippage in schedules in the course of a program. Although the mean of the performance factors was about 1.0, the factors extended over a range of about 0.5 to 2.0. The frequency distributions of these three elements for the 1960s systems are indicated in Fig. 1. The cost, schedule, and performance factors are displayed so that all points plotted to the left of unity represent outcomes better than predicted, and all points plotted to the right of unity represent outcomes worse than predicted.**

A comparison of the cost, schedule, and performance factors suggests that performance was treated as the dominant system development objective and that schedule goals held a very slightly lower priority.

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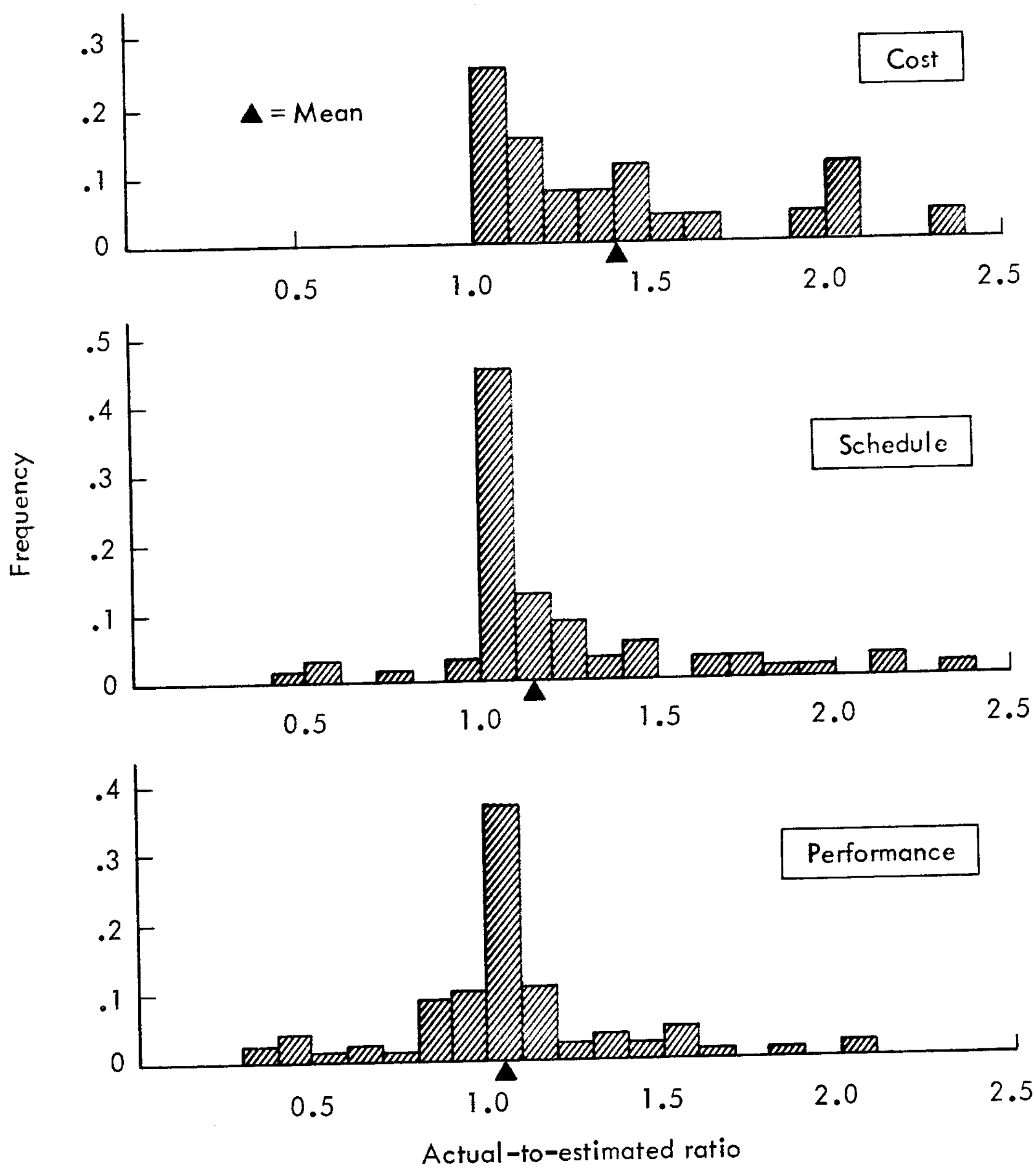
*Questionnaire returns for 20 of the systems in the original sample indicated a mean development cost factor of 1.92 and a range from 0.98 to 5.57. However, there is great imprecision in the responses, arising largely from data inadequacies and definitional problems.

**Some performance ratios were inverted in order to make them consistent with the above description. This was necessary since, in the case of performance factors, a desirable outcome sometimes results in an actual/estimate ratio being greater than unity (for example, missile range).



The frequency distributions have been normalized so that if the heights of the bars for any factor are totaled, the sum will be unity. The vertical axis gives the proportion of factors that fall within any given interval. The orginal data provide more schedule and performance factors than cost factors. This anomaly is explained by the fact that a variety of milestones, in addition to initial operational delivery, were used in the schedule analysis. For the performance analysis, the project officers who provided the original data were asked to identify four or five principal performance parameters crucial to their systems. This was necessary to permit inclusion of performance parameters for a wide variety of systems.

Fig. 1—Frequency distribution of cost, schedule, and performance factors

Cost increases seem to have been accepted in order to meet performance and schedule goals.* There are two sources of support for this interpretation. First, on the average, performance objectives were met while schedules were allowed to slip somewhat and costs even more. Also, it is clear from the entire distribution of outcomes that performance was frequently attained or exceeded; to a lesser extent the same is true of schedules; but costs have invariably been higher than predicted.

The program outcomes of the 1960s reported in Table 2 were compared with a data base of 21 Air Force aircraft and missile programs of the 1950s. These data were originally compiled by Eugene Brussell of Rand between 1957 and 1961. They were initially used in weapons acquisition studies by A. W. Marshall and W. H. Meckling in 1959** and subsequently by B. H. Klein and R. Summers in a series of studies completed in the early 1960s.*** Parallel but entirely separate studies of the characteristics of the weapon system acquisition process of the 1950s were conducted by M. J. Peck and F. M. Scherer of Harvard University in the late 1950s and early 1960s.**** Basically, it is the findings of these studies that have most often been cited by critics of the present acquisition process in attributing highly inefficient development practices to the 1950s and in assuming that the practices

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*See Perry *et al.*, *System Acquisition Experience*, RM-6072-PR, November 1969, for a more detailed discussion of performance factors.

**A. W. Marshall and W. H. Meckling, *Predictability of the Costs, Time, and Success of Development*, The Rand Corporation, P-1821, December 1959, also in *The Rate and Direction of Inventive Activity*, Princeton University Press, Princeton, 1962.

***Burton H. Klein, "The Decision Making Problem in Development," in *The Rate and Direction of Inventive Activity;* Robert Summers, *Cost Estimates as Predictors of Actual Weapons Costs: A Study of Major Hardware Articles*, RM-3061-PR, The Rand Corporation, March 1965; also in T. Marschak, T. K. Glennan, Jr., and Robert Summers, *Strategy for R&D: Studies in the Microeconomics of Development*, Springer-Verlag, New York, 1967.

****M. J. Peck and F. M. Scherer, *The Weapons Acquisition Process: An Economic Analysis*, Harvard University Press, Cambridge, Massachusetts, 1962; and F. M. Scherer, *The Weapons Acquisition Process: Economic Incentives*, Harvard University Press, Cambridge, Massachusetts, 1964.

followed in certain prominent programs of the 1960s were at the same general level of ineffectiveness. In all cases the earlier researchers were concerned with cost factors.

All of the researchers acknowledged two difficulties that in some respects limited the usefulness of their findings. First, in the 1950s (and particularly in the early 1950s) analytical methods of estimating probable program costs were in a very primitive stage of development, and cost estimates therefore tended to be imprecise. A further difficulty was that many of the systems to which the cost estimation techniques were applied in the 1950s were more complex and more technically difficult than the systems used as a data base for early cost estimation models -- the systems with which the Air Force had experience shortly after World War II. Comparisons were awkward and therefore not always acceptable to critical audiences. A second major complication was the difficulty of obtaining initial estimates that were consistent with one another. There was no single point in the programs of the 1950s at which a cost estimate was systematically reviewed and in some sense approved by officials who were responsible for authorizing the start of system development. Researchers had therefore to attempt to reconcile several sets of estimates made at various times throughout the planning and development phase of each program. Cost factors for the 1950s programs are presented in Table 3.

The methodology used in comparing cost factors of one decade with those of another decade is fully documented in a recent Memorandum* so only a few salient features need be summarized here. The most important element of the analysis was the introduction of a measure of technical difficulty. Early in the course of conducting this research it became apparent that some measure of the technical difficulty of individual programs had to be included in order to make meaningful comparisons among programs of markedly different technical scope. For the purposes of the initial analysis, the programs in the 1950s sample and the programs in the 1960s sample were rated on a scale ranging from 1 to 20, the numerical value in each instance being intended to

*Harman and Henrichsen, RM-6269-ARPA.

Table 3

DEFLATED COST FACTORS -- THE 1950s SYSTEMS

System	Factor	System	Factor	System	Factor
F-102/106	4.06	B-58	4.00	IRBM Thor	1.33
	2.46		5.10	Snark	3.10
	1.49		3.64		1.17
	1.30		1.95		1.53
	2.35	B-52	2.62	ICBM Titan	1.00
	1.30		1.33	ICBM Atlas	.77
F-101	.57		1.44		.82
	1.12		.97		1.32
F-100	1.20		1.46		.85
F-94C	2.56		.87		.88
F-89	2.04	B-47	1.25	Falcon	2.52
	1.52		.70		3.80
F-86D	.78		1.55		2.09
F-86A	.93	C-133	1.55		.51
F-84F	2.02	C-130A	1.49	Bomarc	7.10
F-84C	1.55	KC-135	.81		5.90
			.92		3.48
			.80		2.22
					1.49
					1.16
					1.00

The listing of multiple cost factors for individual programs indicates that different "initial estimates" were made at various times between program conception and program completion. The method used to reconcile such data is described in Harman and Henrichsen, RM-6269-ARPA.

represent the magnitude of technical advance that was sought at the time of program approval. In the initial research such values were assigned subjectively, based on assessments obtained from members of the Rand staff who were cognizant of the characteristics and attributes of individual weapon systems. (Ratings for the 1950s systems were based on a survey conducted by Eugene Brussell and employed by Marshall and Meckling, Klein, and Summers.) This numerical value, which became known as the A-factor, was introduced in the cost factor model developed by Harman.

In an effort to expand the A-factor data base, a larger number of experts, both within and outside of Rand, in this country and abroad, subsequently contributed subjective ratings for a rather large number of systems characteristic of the 1950s and 1960s. The subjective ratings in the expanded sample are reproduced in Fig. 2.* As a means of distinguishing between the various categories of technical advance sought at the onset of development in a given program, generalized definitions were adopted for nine categories ranging from 2 to 18 on the numerical scale.

The other principal determinant of cost factor differences between programs, as indicated at this stage of the research, was program length. For the purposes of this analysis, an effort was made to measure the duration of a program between the point at which the service was authorized to proceed with full-scale weapon system development and the point at which the development was essentially completed. However, differences in definition among the services and the rather wide variety of systems with quite different operating and production characteristics included in the sample made it advisable to avoid the end-of-program indicator most frequently used in past studies of this sort: initial operational capability (which ordinarily means the point at which an operationally useful quantity of systems has been delivered to the using service). Instead, the dating of the development program "end"

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*The data shown were accumulated during several surveys conducted over many months, and the procedures varied in detail from time to time, so that all data points may not be strictly comparable.

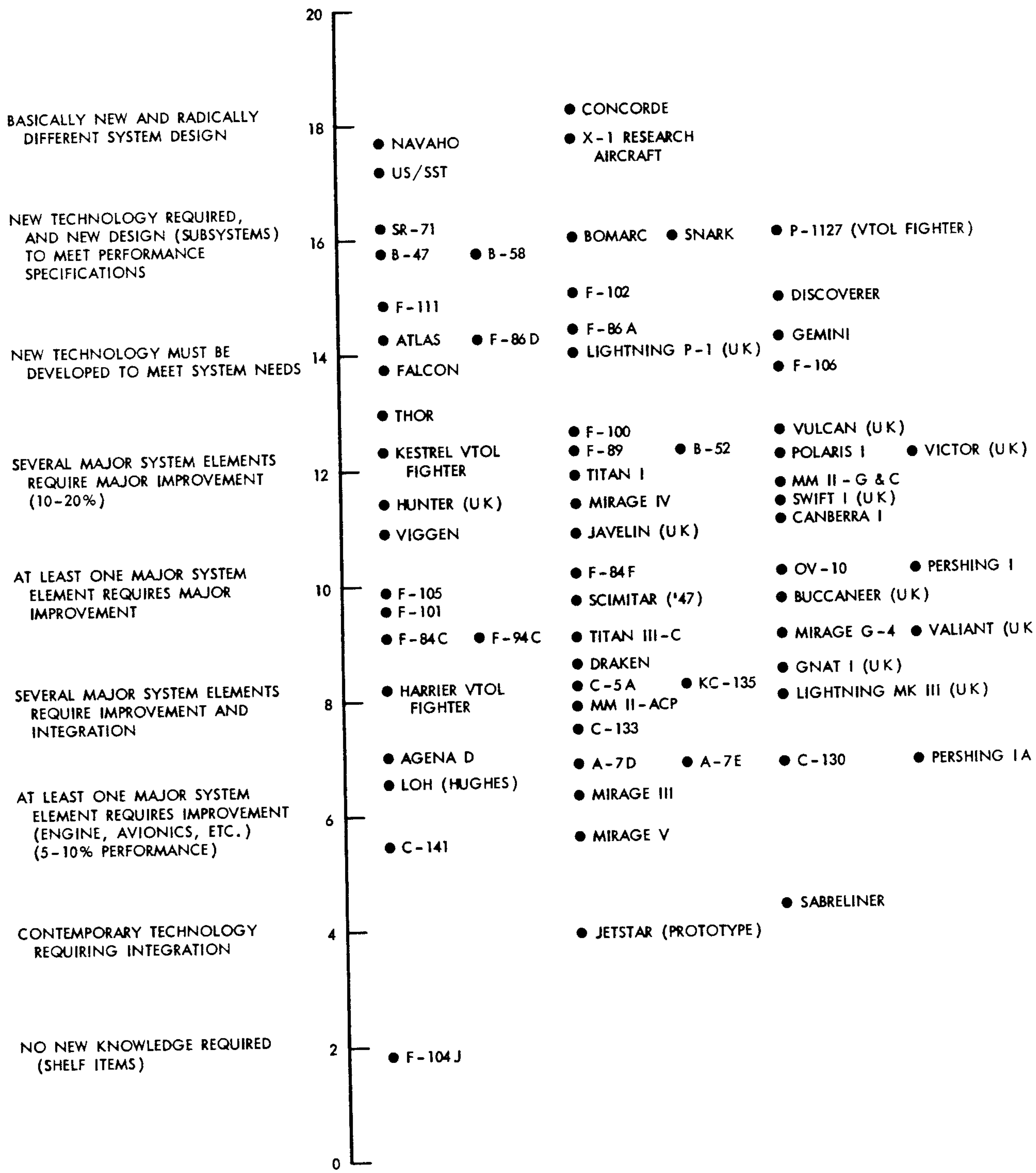

Fig. 2—Technological advance ratings

was taken to be at the time of acceptance of the first operational item delivered to the using service.

It does no great violence to conventional wisdom to suggest that programs with a high content of technical risk and of considerable duration would be more difficult to predict and control than straightforward short-term programs. The analysis of programs spanning two decades transforms such conventional wisdom into supportable findings. Longer programs and those requiring larger technological advances usually have higher cost factors, indicating poorer predictability of program outcomes for such systems.

A product of the analysis was the conclusion that the prediction and control of system acquisition programs had not appreciably improved in the 1960s over the 1950s, although program outcomes tended to deviate less from program predictions in the 1960s because lower technological advances and shorter programs were characteristic of that decade. That point may be illustrated by observing that the typical A-factor value of programs of the 1950s was 12.2 while the average A-factor value for the 1960-decade systems in the sample was 8.9. Such a comparison suggests very strongly that the principal effect of the program review processes introduced in 1961 and honored thereafter was to screen out some of the higher risk programs that might have been approved for development in an earlier era, with resulting extreme departures from predicted outcomes.

It is also apparent that when a full account is taken of the program characteristics mentioned above, the popular impression that programs of the 1950s tended to experience cost growth of 200 percent or more in the ordinary course of events may not be an accurate representation of what actually occurred. The distribution of outcomes for the 1950s is, as revealed in the tables above, somewhat broader than for the 1960s. But for programs of comparable length and technical difficulty, differences in program outcomes for the two decades are not statistically significant.* Further, given the assumption that programs

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*For additional details on the comparison between the decades, see Harman and Henrichsen, RM-6269-ARPA.

of the 1970s will be conducted very much along the lines of those of the late 1960s, future cost uncertainty will still be rather large, especially for programs that will require major technological advances.

II. THE SOURCES OF PROGRAM GROWTH AND THE PREDICTABILITY OF PROGRAM OUTCOMES

RECENT EXPERIENCE

Examination of the development histories of several major Air Force programs provides a general explanation for the causes of cost growth. The three broad candidates for cost growth responsibility were identified earlier: technical uncertainty, scope change, and cost estimating error. Obviously, inefficiencies in program management or the selection of an inappropriate program strategy would tend to be captured by one of these three broad categories. About one-third of observed cost growth and much of the deviation of system performance from that initially anticipated appear to be attributable to technical uncertainty. So also is some small part of the observed schedule slippage. However, approximately one-half of observed cost growth and an additional part of schedule slip appear to be directly attributable to scope change (that is, changes in program objectives imposed on the program after the start of weapon system development). The residual cost growth can be attributed to estimating inaccuracies.

In an era of declining budgets, the consequence of spending a great deal more money than had originally been planned for a given program may be a reduction in the quantity of items ultimately purchased for the inventory. Characteristically, in the 1960s the total funding requirements projected at the onset of a program have been exceeded only modestly; when development costs and unit costs proved to be higher than anticipated, the need for increased total funding was often avoided by a cutback in production quantities. The negative consequences of this practice for planned force structure are obvious.

Of the sources of cost growth cited above, estimation inaccuracies have the least impact. Only about 15 percent of the observed cost growth (the residual mentioned above) can be assigned to the inherent imprecision of present cost estimating procedures. That is not a small error. Nevertheless, if this general distribution of the sources of program deviations is valid (and no evidence that strongly contradicts

these findings has yet been unearthed), reducing the inaccuracy of present cost estimating procedures by half, which by general consent would represent a very substantial improvement, probably would have no clearly detectable effect on the predictability of program outcomes.*

The effects of scope change are sometimes difficult to distinguish from the effects of changes resulting from unanticipated technical difficulties. However, an examination of the details of several major programs indicates that scope change generally entails levying more demanding performance requirements on individual systems and that such change ordinarily imposes additional costs on an acquisition program. It is necessary to recognize, however, that it is in the nature of major programs to change objectives as time passes; requirements are refined to reflect new or updated threat assessments, alterations in national strategy cause mission requirements for individual systems to expand, and new and promising technology influences systems in development. Thus, for example, the complexity of avionics installations and the requirement for greater avionics responsiveness to a larger variety of operational situations tend to induce a regular growth in demands for avionics performance as a program proceeds. Accuracy and reliability requirements for missiles, aerodynamics performance parameters for aircraft and helicopters, and similar performance functions of other weapon systems are frequently made more demanding. Such alterations in program objectives account for the majority of scope change effects.

The influence of the third source of cost growth, technical uncertainty, is illustrated by Fig. 3, which shows the dependence of cost factors on program duration and technical difficulty. This figure displays the cost factor plotted against program length, for three ranges of technological advance.** Programs having A-factor values

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*Notwithstanding this indication, work on improving the accuracy of cost estimating processes still is in progress, both at Rand and elsewhere. If other sources of program growth can be identified and controlled, then the accuracy of the cost estimating process could well become a more significant consideration.

**See Harman and Henrichsen, RM-6269-ARPA for details of the regression analysis.

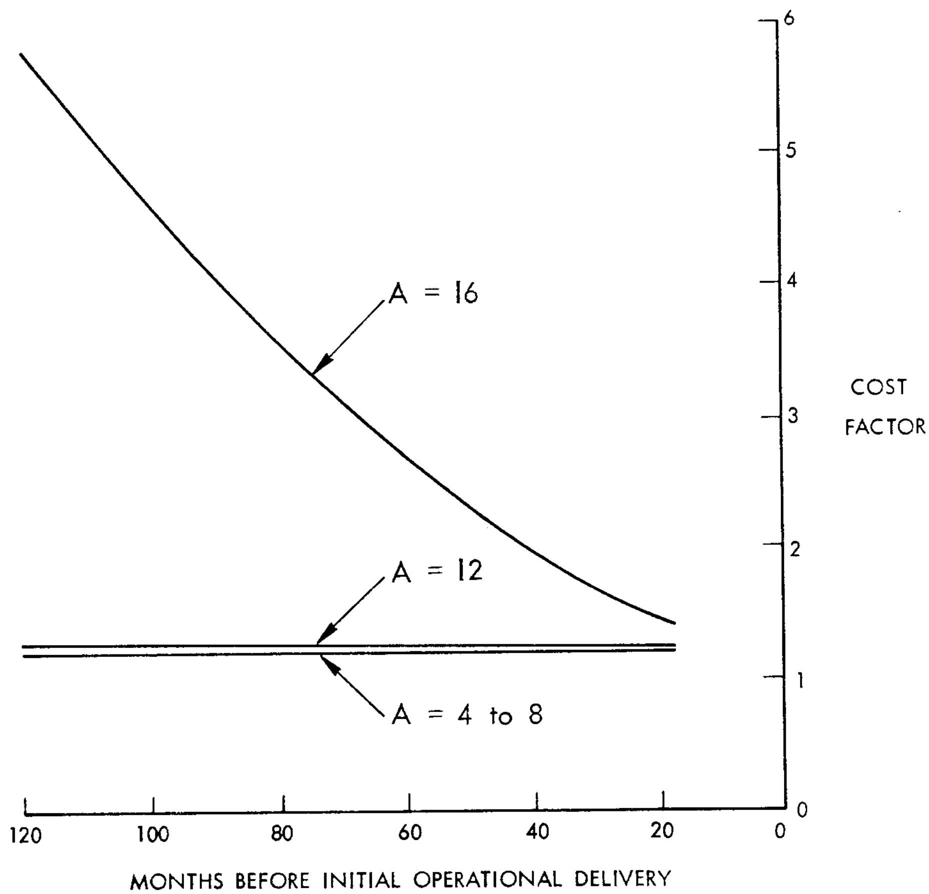

Fig. 3—Variation of cost growth with program duration and level of technical difficulty

of less than about 12 tend to be reasonably predictable, although the distribution of outcomes begins to widen for programs with A-factor values larger than 8. For programs with A-factors above 12, the predictability of program outcomes lessens appreciably, and the worsening effect is not on a linear scale. As the A-factor value increases and program duration extends past 60 months, the cost outcome of a given program becomes so unpredictable that initial cost estimates are very nearly worthless.

The model could also be used to indicate the probable extent of cost growth for future systems. The implication of the initial research was that if the A-factor of a new system could be accurately assessed and appropriate account taken of it at the time estimates were made, the extent of program uncertainty might reasonably be postulated.

Given the assumption that future programs being assessed will be planned and conducted very much in the fashion of the programs in recent experience, from which the data are drawn, it is theoretically feasible to predict the probable cost growth of a program in terms of its duration and the degree of technical advance it encompasses. First, it is necessary to assess the level of technical difficulty encountered by weapon system development programs in the past. Such an assessment can be applied to entire systems or to subsystems. Then, as a second step, it is possible to measure the total program cost growth and the typical schedule change incurred in relevant past programs and to relate that measurement to the assessed technical difficulty of a specific program being analyzed. Using the cost factor model described in Section I as a basis, the probable cost growth of a proposed system (measured by its cost factor) can then be predicted.

Such a process can aid in anticipating prospective cost growth only to the extent that the development strategy and management style of the program being examined are fundamentally similar to those represented in the data base. Were a substantially different strategy used for future programs, as might occur if new approaches to weapon system acquisition were required by DoD or forced by budgetary pressures, the

procedures described here for estimating program outcomes could become unreliable. Introducing parameters representative of each of several basic strategies might tend to compensate for such uncertainty, but as yet there is no way to define such strategy parameters with precision.

A second consideration when using such a procedure in a predictive mode is the irresponsibility of assuming that the cost estimate for a given system should be adjusted upward in some arbitrary way solely because the system being examined has a "high" A-factor value. *Apparent* cost growth would be limited by such a strategy, but the goal of most program managers is to *control* cost rather than to minimize apparent cost growth. Further, the implication of a "high" A-factor may be that the system being analyzed will cost more -- and perhaps a great deal more -- than generally comparable programs. Yet, neither the original cost estimate nor a cost estimate adjusted by an A-factor corrective is necessarily a "should cost" estimate. Rather, each indicates *probable* cost, given certain basic assumptions about program length, the technological content of the program, and acquisition style. Should any estimate, however obtained, strongly suggest that program costs will be unacceptably high, an appropriate response would be to change one or more of the variables -- program style, or level of technical advance sought, for example. Alternatively, if other variables must remain constant, for any of several possible reasons (such as urgency of the threat), it would be no more than ordinary common sense to acknowledge that program costs may be very high -- and to adjust budgets accordingly. It is reasonable also to infer that a reduction in the value of a technical advance factor could be obtained by demonstrating in some relatively inexpensive way that the desired technical advance could actually be achieved. Finally, there is no evidence that imposing additional management controls of the sort contrived to counteract some of the unfavorable program outcomes of the 1960s will appreciably reduce the probability of an undesired program outcome in the 1970s.

A parametric cost estimating model that includes an A-factor as one of the variables has several attractions. But before an attempt is made to develop such a model, it would be advisable to generate

A-factor values that are not highly dependent on the candor and objectivity of the estimators -- by some technique not subject to the appraisers' knowledge of past program outcomes. The underlying assumption of A-factor analysis is that the assigned value reflects an objective appraisal of the technical advance actually sought at the time the program began. Thus some improved measure of the range of technical advance sought is greatly to be desired.*

SOME PROCEDURAL APPROACHES TO CONTROL OF COST GROWTH

Among ways of decreasing the probability of large cost growth in new programs are some potential procedural changes of rather limited scope. The first of these is to modernize and somewhat improve the accuracy of present cost estimating models. As mentioned above, however, even substantial improvements in such models will not significantly improve the predictability of costs. A second approach might be to increase the emphasis on the cost elements in contracting. However, conventional ways of utilizing incentives for the cost elements in system contracting do not appear to hold much promise.**

It is possible, at least theoretically, to identify early indicators of impending design problems during development. An increase in the frequency of engineering changes, a mid-program shift to newer and more exotic materials, an increase in the number of engineers devoting attention to some technical area, or any of several other "symptoms" might be used in this fashion. Evidence that such evaluation has operational utility in the early detection of design and engineering problems is lacking, however. One difficulty may be that current data reporting systems are insensitive to such indicators --

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*The possibility that useful A-factor values can be analytically obtained has been explored on a modest scale since mid-1970. Although this line of research has promise, no conclusive findings have yet emerged.

**I. N. Fisher, *A Reappraisal of Incentive Contracting Experience*, RM-5700-PR, July 1968. Some additional and more recent work along these lines indicates that the differences in program outcomes that can be attributed to differences in contract types are for practical purposes undetectable.

and there is a considerable uncertainty about what are the *real* indicators.*

One of the difficulties of relying on development program trouble indicators is that they can be highly unreliable if either the contractor or the program manager wishes to disguise his problems in the expectation of being able to resolve them before they attract attention. Reliance on technical milestone indicators of the sort that have been used to "validate" progress in recent programs may be unwise. Indeed, the practice of awarding incentive payments because a program has passed a technical or demonstration milestone may in some cases encourage a contractor to demonstrate some feature on a one-time basis even though the real technical capability nominally demonstrated may not be routinely or regularly achievable at all. Measurements of sustained performance (for example, the 50-hour operating requirement imposed on jet engines) may represent the only reliable milestone indicators of technical progress.

Finally, in the category of improved program control procedures that might be invoked without materially altering present program structures lies the possibility of devising an improved method for handling scope changes. Evidence obtained from case histories and from examinations of scope changes that have affected recent programs indicates that the impact of such scope change on program outcomes is not ordinarily assessed on a total program basis. Only recently, for example, have significant cost constraints been placed on the engineering change process. Although there would appear to be a considerable area for improvements in the control of scope change, minor changes in management procedures are unlikely to create any major improvement in outcomes.

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*The hypothesis that probable program growth can be foreseen toward the end of initial hardware test phases is being examined as one possible way of identifying impending program growth.

III. SOME ALTERNATIVE ACQUISITION STRATEGIES

If the probable outcome of a 1970s program conducted in the pattern of typical programs of the 1960s is likely to be unacceptable to the sponsoring service, the Department of Defense, the Congress, and the general public, is it reasonable to suggest that alternative approaches to the acquisition process may provide some relief? An answer may lie in examination of recent development programs that did not conform to "typical" patterns. There are several obvious candidates for research in this area. One includes Department of Defense programs exempted from the ordinary controls imposed on acquisition processes of the 1960s. A second category includes a range of development programs conducted in Europe in the last ten years.*

In the category of programs exempted from the ordinary controls imposed by the Department of Defense system acquisition process generally favored in the 1960s, it is difficult to find examples that can be cited on an unclassified basis. However, one extremely interesting case is the Agena D.

AGENA D

Early in 1961, increased activity in various U.S. space programs and dissatisfaction with the performance and cost of available space vehicles induced the Air Force Space Systems Division to consider means of reducing costs and increasing launch vehicle flexibility through standardization of what was then called the Agena B. In June of that year, the Space Systems Division authorized Lockheed to begin the design

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*Another possible candidate is civil aviation programs. The obvious area for inquiry is the development of large commercial transport aircraft. Rand has completed relatively little work in this area; very preliminary research suggests that there is cost growth on the order of at least 10 percent and schedule slippage of 10 percent to 15 percent. There are also performance shortfalls -- perhaps as large as 10 percent. The differences between actual outcome and original plan are clearly smaller for commercial aircraft than for their military counterparts, but no striking differences in performance requirements or technology that explain the differences in program outcomes can be readily identified.

of a standardized Agena space vehicle and to submit a proposal for its development and test. Lockheed responded in July, and on 25 August accepted an Air Force letter contract committing the firm to develop and produce what was to become known as the Agena D. The desired vehicle was to be used in a number of different space programs with minimal modification; to lessen development time and limit reliance on new technology, Lockheed was to use available Agena B technology as extensively as possible. The contract provided for the delivery of twelve flight vehicles and a static test article, the creation of a capability for producing five vehicles each month, and the establishment of a price for follow-on procurement. The cost of the development effort and the required vehicles was estimated to be about $60 million. First launch was tentatively scheduled for late January 1963.

Early in October 1961, in the aftermath of a briefing on standardizing the Agena, the Undersecretary of the Air Force concluded that an accelerated schedule and perhaps a lower development cost might result from adopting an approach other than that initially proposed. He appointed a special committee headed by C. L. Johnson, Lockheed's Vice President for Engineering, to "investigate ways and means of providing a more reliable Agena on an accelerated schedule." Over a period of ten days the Johnson committee reviewed the Agena proposal and Lockheed's capacity for accelerating the approved schedule. The committee concluded that a reliable standard Agena could actually be developed and launched by June 1962 at a somewhat lower cost than initially proposed, provided that *unusual* technical and contractual relationships were accepted by both the contractor and the government. The Johnson committee proposed applying techniques derived from Johnson's experience with his "skunk works" shop at Lockheed's Burbank plant. To insure compliance by both the contractor and the government, ground rules derived from that experience were made a preamble to a contractual work statement for an accelerated Agena D program.

The Undersecretary approved the recommended approach and directed that appropriate work statements, ground rules, and contractual provisions be drawn up. A preliminary agreement was signed in a matter of days. By 20 November, an independent program office reporting directly to the Commander of the Space Systems Division had been established, as had a counterpart organization at Lockheed, Sunnyvale. When the details were sorted out, the Air Force project officer estimated that total funding requirements would approximate $32.9 million dollars.

The resources actually expended by Lockheed in carrying through the agreement are summarized in Table 4. The "development test vehicle" was actually completed by the end of March 1962 and was immediately used to qualify components peculiar to the various applications proposed for the Agena D. The first operationally capable Agena was delivered on 16 April and was launched on 27 June. By that time, all four in the first lot of Agena Ds had been delivered (on or before their scheduled due-dates), and the remaining vehicles were on schedule.

A standardized production configuration was adopted and approved by the Air Force in September 1962, and production of a larger lot of vehicles began immediately after completion of the first set of twelve. Of the first 24 Agena Ds launched in a variety of programs, 21 were successfully injected into orbit. Total program costs for the original twelve-vehicle effort, including maximum incentive fees on performance, schedule, and cost elements, came to $31.7 million, of which more than $2 million represented fees and the like. The engineering work needed to satisfy the initial development objectives proved to be about one-fourth of that originally postulated; the number of quality-control personnel involved was reduced from an anticipated 1200 to a total of less than 70; and tooling costs were reduced by an order of magnitude -- from a predicted $2 million to $0.15 million. Project staffs, both in the plant and the SPO at the Space Systems Division, were notably small. Although technical reporting was minimal, it was entirely adequate when judged in terms of work accomplished. Remarkably, only about 10 percent as many engineering drawings were required for

Table 4

THE AGENA D DEVELOPMENT PROGRAM

	As Planned	As Done
Cost	$60 million	$32 million
Time	18 months	9 months
Drawings	3900	350
Engineers (factor)	4	1
Quality control personnel	1200	69
Product improvement program	Yes	No
Tooling	$2 million	$0.15 million
Drawing release lag	30 days	1 day
Technical progress reporting	"Normal"	"Minimal"
Project office staff in plant	?	4
System Project Office (SPO)	?	25

the twelve-vehicle Agena D program as would ordinarily have been demanded for a program conducted by the rules and procedures then customary. The success of delivered vehicles in meeting program objectives was indisputable; by a considerable margin, the Agena D was both more mission-adaptable and more reliable than its Agena B predecessor -- and it cost some 30 to 40 percent less. Reliability was at least twice and perhaps five or six times that of the Agena B.

In general, the working interface between the Air Force and Lockheed and the effectiveness of the relationship were appreciably better in the view of most observers than had been true of earlier work of the same general nature. Almost precisely the work projected in the original proposal had been completed, the cost appeared to be about one-half of that originally anticipated, schedules were compressed, and the performance of the delivered vehicle was appreciably superior to that of its predecessor. In the terms ordinarily used to evaluate a program of this sort, the Agena D development effort is difficult to fault.*

WEST EUROPEAN EXPERIENCE

Two of the authors (Perry and Smith) visited several major European aircraft firms in the spring of 1970 to investigate the procedures and achievements characteristic of recent aircraft development efforts. They obtained data on a number of individual aircraft projects from four principal European manufacturers. Two were British, Hawker-Siddeley and the British Aircraft Corporation; the French firm was Avions Marcel Dassault; SAAB, the only Scandinavian firm now developing and building military aircraft, was the Swedish. Table 5 reproduces a selection

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*This description is based on an unpublished Rand case study of the Agena D development program by E. O. Johnson, compiled from documents in historical files of the Air Force Space and Missile Systems Organization. Two important factors of the foregoing summary should be noted: first, the later Agena D production program did *not* follow the pattern of the original development effort; second, the Agena D rather than earlier or later versions, or Agenas used by NASA, is the topic here.

of data obtained during the trip. For the most part, the data were furnished by the aircraft builders, although the British Ministry of Technology and Ministry of Defense, and the Aircraft Materiel Department of the Swedish Ministry of Defense provided corroborative and supplemental information covering aircraft of those countries.

A simple comparison of maximum attainable speeds for fighter aircraft, as shown in the first line of Table 5, is of course not a complete indication of aircraft performance; but it serves to illustrate that the general level of technology exploited by major European aircraft builders in the 1960s was not significantly inferior to the level of technology achieved by major American aircraft builders.* Of the six European aircraft chosen for illustrative purposes here, three have held world speed marks of one kind or another; the Harrier is the only operational vertical takeoff fighter currently in use anywhere in the world; the Viggen is a unique canard-configured fighter with versatile performance; the Mirage-III and derivatives of it have been the basic fighter aircraft of the Israeli Air Force and have been used widely in Middle East combat for several years; and the Mirage-IV is a 70,000 pound aircraft with a performance not greatly different from that of the F-111. Certainly there are performance differences, structural differences, requirements differences, and configuration differences among these aircraft and between such aircraft and those characteristically used in the United States for the past eight or ten years. But, in general, current products of European aircraft technology are apparently not highly dependent on the products of American research and development or American experience (although both are frequently asserted), nor are there any striking technological differences between the products of European aircraft construction practices and American aircraft construction practices today. Differences exist (and in some cases they may be extremely important), but they do not appear to be

*It is perhaps worth note that European technology is represented in several contemporary U.S. military aircraft; the engine and "heads-up display" of the A-7, the wing box of the variable-sweep F-14, and the Harriers purchased by the U.S. Marines are well known examples.

Table 5

AIRCRAFT DEVELOPMENT EXPERIENCE
(West European and U.S.)

	Harrier (Kestrel)	Jaguar (B.A.C.)	Viggen	Lightning (P-1B)	Mirage IIIA	Mirage IV	F-111A
Performance (Mach)	.9 (VTO)	1.6	1.8	2.2	2.2	2.2	2.3
Manning:							
Engineering staff	160 to 330	320	650	400	50	70	4000 to 6000 Total
Experimental shop	350 +	300	350	300	300	400	
Program office (Government)	24	35	20	20	10	12	220
Months from design start to:							
First flight	22	54	43	34	16	17	25
First production item delivered	48	64	96[a]	45	38	54	58
Number of test vehicles	13	6	6	5	3	4	23
Development cost factor	1.0	1.1	1.3	1.4	1.0	1.1	1.8

[a]Includes 24-month schedule stretchout to reconcile a changed threat estimate with altered budget constraints.

much greater than the differences among various models of American combat aircraft of the 1960s. The most notable difference between typical European and American operational aircraft of 1970 is in the quantity and complexity of installed avionics equipment. To minimize the influence of that factor, all of the data in Table 5 concern the basic flight vehicle and exclude the weapons-oriented systems for each.

The last horizontal line on Table 5 lists development cost factors of the sort discussed in preceding sections. Two points need to be made in considering them: first, actual development costs are not readily obtainable for U.S. aircraft owing to our accounting practices, whereas in Europe the separation of production from development tends to make development costs more or less determinable; second, the cost factors for aircraft shown in Table 5 generally exclude avionics development. Development cost factors for U.S. *systems* (including avionics) range from 1.5 to more than 2.0. For the F-111, the example chosen here, the cost factor for airframe-engine development (excluding avionics) is *at least* 1.8. Inclusion of avionics development costs, as best they can be estimated, drives the development cost factor to at least 2.0. In recent European programs examined by Rand, the typical development cost factor appeared to be about 1.2 or 1.3, and in the sample examined there was no development cost factor larger than 1.4. Because definitive information on the absolute cost and estimated cost of recent European aircraft development could not be readily obtained, the cost factors for European aircraft were provided by government offices or contractor organizations who used Rand's definitions and ground rules. It is conceivable that the numbers are biased, but cost information independently obtained seems to corroborate that provided by the Europeans. Even if the numbers are wrong by 20 or 30 percent, they still are revealing. Perhaps more important, the costs for aircraft development evidently are substantially lower in the European case.

The period typically required to proceed from design start to either first flight or to the availability of a first production item is not significantly longer in Europe than in the United States. The

Dassault case,* which has been widely discussed in the United States recently, is truly exceptional. On one occasion, Dassault progressed from design start to the first flight of a prototype aircraft in nine months; that achievement, which occurred in the late 1950s, has not been bettered in the United States since 1944, when the P-80 went from design start to first flight in six months. In the case of the Mirage IV, the development program was marked by the same sort of uncertainty that has characterized several high-risk American programs. For example, at the time design began Dassault did not know with any certainty the required size of the bomber or the weight or bulk of the first French nuclear weapon. Dimensional assumptions passed to Dassault by the French government ultimately proved to be invalid, and the second engineering test aircraft had to be made appreciably larger than the first. There was an additional complication in the fact that the engine preferred by Dassault was a license-built version of an American engine, and the French Air Ministry was extremely reluctant to adopt a propulsion system that might be somehow dependent on the unpredictabilities of American international policies. Consequently, a French engine was installed midway through the development program and the desired performance was adversely affected.

Aircraft first delivered to the French Air Force for use in the *Force de Frappe* had, therefore, about 10 percent less performance than planned. Dassault and the French Air Force maintain today that the performance disparity has been overcome and that inventory aircraft are for practical purposes comparable in performance to those specified when the design program began. Dassault aircraft -- at least the Mirage series -- are evolutionary. There is no disputing the fact. But the 70,000-pound Mirage IV obviously could not have been scaled up from a Mirage III, which weighed less than 25,000 pounds fully loaded, without considerable design ingenuity and originality.

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*Illustrated in Table 5 by the Mirage III and Mirage IV, but including several other aircraft in the Mirage family.

Among the most interesting data items obtained in the recent survey of European aircraft development were the manning numbers for the engineering staff, the experimental shop where engineering test vehicles were constructed, and the government program offices or their equivalents. Fewer engineers were employed in the design process than in comparable American design staffs, in some cases by a factor of ten and in almost every case by a factor of three or more. The numbers of supervisory and management personnel assigned to the government project office appear to be smaller by about the same ratios. The technological and performance differences that distinguish the European aircraft in this sample from comparable American aircraft are in no sense large enough to explain the striking differences in invested manpower resources.

In some part the explanation lies in the fact that for nearly ten years most European aircraft developers, and their governments, have been operating under severe financial constraints. Also, there simply are not enough aircraft engineers available in any West European nation to man a program of the sort that is ordinary in the United States. For practical purposes the Europeans have compensated for this disparity in available resources by pruning away the redundancies of the development process. In the case of Dassault, there is no periodic reporting of progress, no milestone reporting, no repeated briefings of any sort for the government. Progress accounting is on an irregular basis and by letter. In the case of the British (for the Harrier project), progress is reported by means of one monthly summary, about a quarter of an inch thick, which contains synopses of all important events, program status evaluation, and program projections. The contractor, Hawker-Siddeley, manages the Harrier program on the basis of a comparable report that contains about 12 or 15 additional pages.

In Western Europe, very few engineering personnel are engaged in the sort of data analysis that is common to American programs. The Swedes indulge rather more extensively than do the British or the French, but even in Sweden the amount of work that is not immediately relevant to the task at hand is surprisingly small. True, some American management devices are widely used; PERT, for example, is employed

by both SAAB and the British Aircraft Corporation. But the PERT schematic ordinarily used in Europe captures no more than 6,000 to 10,000 events. A short and uncluttered technical decision net is characteristic. No more than three or four approvals are required -- all at the working level -- to validate drawings; project managers have direct access to the two or three people whose approval is necessary to fund design changes or to approve them in terms of the performance implications for the system.

The fundamental strategy used widely in Europe, although not in all programs, of course, calls for no substantial production commitment to be made until the basic development process has been completed and proof of utility demonstrated by performance tests of an engineering shop article that is reasonably representative of the desired operational item. The common terminology is "prototype," but in actual practice what is done is strikingly different from the sort of activity usually implied when the term "prototype" is used in the United States. In particular, engineering shop aircraft are ordinarily assembled on what would be considered "hard" tooling in this country -- but not a full set of hard tooling. The Viggen is a remarkable case in point. To complete fabrication of six engineering shop Viggen aircraft, SAAB spent 25 percent of the total tooling budget for the Viggen program. At the end of the engineering-shop phase of development, when fabrication of operational aircraft was approved, about 10 percent of the tooling used in fabricating the first six aircraft had been discarded or replaced. That 10 percent throwaway represented only about two and one-half percent of the total tooling budget for the Viggen; and in that instance the tooling change accommodated a relatively substantial alteration of the basic fuselage airframe and appreciable changes in both the lift and control surfaces.

In lieu of full hard tooling, Dassault, SAAB, BAC, and Hawker-Siddeley rely on highly skilled, permanent-cadre engineering shop personnel to assemble the experimental shop aircraft to the same sorts of detailed drawings that are ordinarily used in this country, but without resort to full sets of jigs and fixtures. Indeed, detailed

tooling drawings are not attempted until there is reasonable assurance that the parts to be fabricated will actually be used in operational aircraft.

Another element of the design process in Europe that is extremely revealing is the pronounced reliance on early proof testing of engines, avionics, and airframes. Production commitments are delayed until both the performance and the probable durability of the subsystems have been appropriately demonstrated.

The small size of the development project staff is an important consideration in the European policy of proof testing systems before committing them to production. A lapse of several months between completion of a test item and commitment to production makes it impractical, on economic grounds alone, to maintain a large project staff while testing proceeds. But a relatively small staff of highly skilled designers can be usefully employed at many tasks and in extreme cases can be "carried" for a considerable period at relatively slight cost. On the other hand, the existence of a large design staff plus production capability common in this country represents a source of pressure to move at once to the next stage of acquisition, be it final design or production, regardless of whether the current development or test phase has been satisfactorily completed.

The delay for validation of performance has another effect; it permits rational and low-cost alterations of specifications to make actual performance correspond realistically to a reappraised threat. The Swedish government accepted a lower level of performance at the mid-stage of development of the Viggen because an anticipated threat had not materialized -- and seemed unlikely to do so in the immediate future.

In general, for about the same level of airframe technology, European schedules tend to be about 10 to 50 percent longer than *predicted* U.S. schedules for comparable aircraft systems, but only 10 to 20 percent longer than *actual* U.S. schedules. The difference represents schedule slips.

In France the cost of military aircraft development through the flight test phase may be but one-fifth and rarely is as much as one-half of the U.S. cost for comparable systems.* Nor does the unit price of production items appear to be appreciably higher than for comparable U.S. aircraft. Further, and perhaps most significant, recent European program outcomes have tended to correspond very closely to the cost predictions made when system development began.** Differences between anticipated performance and actual performance, when they occur, generally appear to have been chosen deliberately as alternatives to incurring costs higher than predicted. But, to restate an earlier caveat, European indulgence in frequent cost-performance tradeoffs should not suggest that the performance of European aircraft is appreciably inferior to that of comparable U.S. aircraft. In most instances, that does not appear to be the case. It is worth recalling that in many respects the performance attributes of American aircraft do not entirely match those predicted when program approval was granted. Nor is the experience of the Department of Defense in requiring and obtaining performance superior to that originally projected exclusive to the United States; improved performance is also observable in advanced models of most European fighter aircraft developments although it appears to be introduced more systematically and somewhat later in the development-production cycle than in U.S. experience.

THE SOVIET UNION

Another indication that strategy differences may have an appreciable effect on the cost and other outcomes of major development programs is suggested by a recent survey of the experience of the Soviet Union in developing aircraft in the decades since the close of World War II. Table 6 lists the numbers of bomber and fighter aircraft

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*Such a statement should not be interpreted as a reflection of an elaborate normalization of wage rates, materials costs, and intensity of tooling. We have not completed such a comparison.

**Obviously, neither the Concorde (Anglo-French SST) nor the Rolls-Royce RB-211 engine fits such a generalization. But they were not undertaken in accordance with customary European practices.

Table 6

U.S./SOVIET AIRCRAFT DEVELOPMENT PROJECTS, 1945-1969

	United States		Soviet Union	
	Total	Entered Service	Total	Entered Service
		Fighters and Attack Aircraft		
1945-49	23	14	35	11
1950-54	18	17	13	5
1955-59	10	8	9	4
1960-64	5	4	6	4
1965-69	2	2	10	5[a]
		Bombers		
1945-49	15	6	10	3
1950-54	8	7	8	3
1955-59	3	3	9	5
1960-64	2	1	3	3
1965-69	1	1	1	?

[a]Three known to have entered service; two additional are "probable."

that the United States and the Soviet Union have carried to the point of publicly observed first flight since 1945. It will be noted that since 1955, a period that basically covers development of all aircraft now in service and those likely to enter service in the next few years, the Soviet Union has carried about one and one-half times as many fighter aircraft to the flight test stage as has the United States, and about twice as many bomber aircraft.

There are two possible explanations for this contrast. One is that the cost of a fighter aircraft development is approximately the same for both countries and that the Soviet Union is spending about twice as much money on the development of military aircraft as is the United States and has been doing so for about ten years. Too little is known about the resources the Soviets have invested in aircraft development in that period to put any confidence in an allegation that such is or is not the case. But relatively recent studies of the Soviet economy and the Soviet budget make it seem rather unlikely that the Soviet Union has indeed been greatly outspending the United States.* And if the Soviet Union now and for some years has been spending no more than the United States on the development of military aircraft, one is driven to the somewhat alarming conclusion that in its initial stages, at least, the Soviet aircraft development process is appreciably less costly than the comparable American development process. It is reasonably clear that the Soviets develop and test aircraft in a way that the United States largely abandoned some 15 years ago; and if evidence of West European experience can be treated as applicable, the strategy favored by the Soviet Union is in many respects less costly than our own. The implication is that the Soviet Union actually has created at equivalent cost more aircraft options than has the United States. The advantage of having several aircraft available for selection (or having them past the proof test stage) and of being able at

*"The Soviet Threat," a summary by the Chairman of the Armed Services Committee, House of Representatives, suggests (p. 9919) that the ratio of defense-related R&D spending since 1955 has been about 6:5, the U.S. spending more. But the statement notes that in 1969 and 1970, the USSR spent somewhat more.

that point to choose the one most appropriate to the imminent threat, seems clear. The available evidence on Soviet R&D practices is far from conclusive; further research in this area seems thoroughly warranted.*

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*Studies of the Soviet design process and its characteristics have been in progress at Rand, on a small scale, for several months. The initial report is by A. J. Alexander, *R&D in Soviet Aviation: The Relationship Between Organization and Outputs*, R-589-PR, November 1970. A report on Soviet science policy is in an advanced state of preparation.

IV. TWO PROPOSALS: AUSTERE DEVELOPMENT AND AN INCREMENTAL ACQUISITION STRATEGY

The analysis of 36 major DoD systems, together with observations of European aircraft development practices and non-standard American programs, identified some major causes of high system cost. The list contains no surprises; these sources of cost growth have been singled out in past studies of the system acquisition process.

High system cost and cost growth appear to arise primarily from efforts to subdue difficult technology on highly compressed schedules and an apparent willingness to pay whatever is required to insure satisfaction of original (or even expanded) system performance goals. Another obvious and important contributor is the customary acceptance of optimistic assumptions about the long-term predictability of technology and the cost of coping with it. Occasionally there may be a valid reason for urgency in satisfying original goals, for incorporating new and more stringent requirements during a program, or for insisting upon original schedule expectations. There is little evidence that extreme urgency characterizes many current programs, however. In most cases the appropriate verdict would seem to be "not proven."

The advocacy of more efficient and more economical aircraft development practices has been a tradition in this country for at least a dozen years. Efforts have been made to identify and eliminate the causes of inefficiency, but such efforts have ordinarily been directed at refining management procedures and exercising better control over the activities of contractor development organizations. Attempts to impel contractors to be more attentive to cost considerations generally have focused on some variant of fixed-price incentive contracting. But neither greater elegance of management procedures nor readiness to force a major contractor into extreme financial difficulties has alleviated the fundamental problem. The success of some recent development and acquisition programs that have been conducted without deference to most of the usual management rituals suggests that a strikingly different strategy for acquisition may be appropriate.

The evidence reviewed in the preceding sections demonstrates three aspects of system acquisition that are too thoroughly supported to be classed as hypotheses but perhaps not sufficiently documented to qualify as truths.

First, despite determined efforts to improve the outcome of major acquisition programs by altering contractual approaches and introducing complex management reforms, recent programs have incurred cost, schedule, and system performance difficulties not greatly different from those characteristic of the 1950s.

Second, although some advantage could be gained by improving the effectiveness of the cost estimating process and by applying techniques that would encourage the earlier identification and correction of the causes of cost growth in individual programs, there is no evidence that *substantial* improvements in the outcomes of system acquisition programs can be anticipated by following that route.

Third, in a number of instances, major system acquisition programs that departed in many respects from the pattern ordinarily imposed by the U.S. Department of Defense in the 1960s had surprisingly good outcomes; proved remarkably predictable with respect to cost, performance, and schedule; and were appreciably less costly in terms of total resources expended than comparable programs carried through by the more ordinary techniques of DoD system acquisition.

These are provocative indicators -- clearly, there is room for improvement in system acquisition policy and there are feasible alternatives. In all candor, we must point out that such evidence as is now available confirms only that there are several possible alternatives that can potentially improve the situation, not that a *uniquely* superior remedy exists. Many approaches may provide relief,* but based on our available empirical evidence, the following strategy seems most likely to facilitate substantial improvements.

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*See also, for example, Blue Ribbon Defense Panel, *Report to the President and the Secretary of Defense on the Department of Defense*, Washington, July 1970 (esp. Appendix E, Staff Report on Major Weapon Systems Acquisition Process).

THE PROPOSED STRATEGY CHANGES

On the strength of evidence derived from an examination of individual DoD programs exempted from ordinary rules of system acquisition, and from evidence provided by examinations of current and recent European aircraft developments, it is possible to construct a consistent set of principles applicable to a wide variety of systems that may be required in the 1970s.

The proposals outlined below represent a sharp departure from the policies common in the 1960s in which normally a single, major authorization decision was made, followed by a highly concurrent program where production was initiated long before development was truly completed. We suggest instead, as the "normal" approach to major weapon system acquisitions for the 1970s, an approach characterized by two key principles: (1) an *incremental* strategy involving a sequence of decision points, and (2) considerable *austerity* in the early phases of development.

An Incremental Strategy

The evidence gleaned from past programs indicates there is much to be gained from conducting acquisition programs in discrete phases clearly separated from one another. Basically, such an approach would require separating the development of systems from the subsequent production of those systems; furthermore, it would call for first conducting those aspects of development aimed at demonstrating the performance potential of the system and later addressing such issues as verifying reliability and maintainability of the system and providing for the special constraints imposed by service support requirements.* Finally, an incremental strategy could, and ordinarily would, include

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*It is assumed here that the design phase included consideration of reliability, maintainability, and similar factors; such is clearly the appropriate and usual practice in most aircraft design processes. Our point is, however, that the preliminary design phase should *not* include an elaborate attempt to resolve maintainability, reliability, and similar issues until there is some reasonable assurance that the system has a performance relevant to need.

periodic reassessment, redefinition, and readjustment of such program constituents as probable cost, performance responses to a changing threat, schedule objectives, cost implications of proposed changes, and residual technical advance required to satisfy program objectives at any given stage of a program.*

The notion of an incremental acquisition strategy certainly is not new. Some variant of it was common in this country twenty years ago, and for practical purposes Europeans have been using it through most of the 1960s. In many instances "exempt programs" have used a similar strategy recently in this country, and it is the ordinary way in which many commercial aircraft are developed.

Major weapon systems can be divided into at least two groups that would benefit from the strategy in different ways. Suppose the A-factor concept described earlier [see Section I, especially Figure 2] is used as a way of defining the groups of weapon systems. As observed [Section II], for systems with A-factors of less than about 12, program growth is reasonably predictable. For programs in that group, the major benefit of an incremental acquisition strategy would be potentially lower system costs, arising from the possibility of maintaining competition to the time of production decision -- either by conducting two or more development programs,** or through competitive initial or follow-on production.*** For this group of systems, it seems unnecessary to schedule major decision points during the development phase; a relatively low A-factor value implies that the performance outcome is reasonably predictable. The Air Force's A-X program, which appears to fall in this group, is perhaps the best current example of applying such a strategy.

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*The technique involves a tradeoff evaluation that balances the cost of obtaining whatever performance has not yet been demonstrated against the urgency of incorporating still-to-be achieved performance in the article finally produced.

**Lack of "austerity" in two or more development programs can easily reduce and possibly eliminate the benefits of competition in choosing the production contractor; see p. 48.

***On some other aspects of the question of separating production from development, see below, pp. 45-46.

The most potentially significant application of an incremental acquisition strategy may be for weapon system developments having A-factor values greater than 12 -- where the current acquisition strategy has generally been unsuccessful. In these cases, tradeoffs between cost and performance, an intentional extension of a development program to provide more time for resolving unforeseen difficulties, or a restructuring of objectives to counter unanticipated threats may be vital to controlling program outcomes. It might even be desirable to terminate a development program if, for example, the technology sought were not attainable at a reasonable price or the threat failed to materialize.* The periodic reassessment implied by application of an incremental strategy suggests recurrent evaluation not only in terms of its intrinsic promise, but also of its advantages over competing systems also in development or already in the force. There has not been a recent example of such an approach, but the strategies used in the period of early ICBM development (leading, ultimately, to the Atlas and Titan) generally resemble those applicable to such high technology programs.

The characteristic response to difficulties encountered in "concurrent" programs of the past has been based on the assumption that more careful advance planning, more thorough analysis, and more extensive preprogram design work would compensate for the uncertainties that troubled earlier programs. Technical uncertainty and the development process have been inseparable companions since long before aircraft and missile development began. In any program marked by a considerable degree of technical uncertainty, it is extremely unlikely that planners can anticipate the precise nature of the difficulties and take steps in advance to resolve them. Careful analysis and planning in advance of program approval should not be abandoned, of course. But planners should not delude themselves into believing that abstract planning, however comprehensive, will resolve technical, scheduling, or cost uncertainties,

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*The difficulties of evaluating a SPO manager under such conditions are not to be minimized. A very *successful* development-only program could occur in which the potential defensive system induced prospective adversaries *not* to deploy a new offensive system.

and that programs will thereafter proceed toward completion without encountering difficulty and without being subject to change in both general goals and small details.

In the course of development of a new weapon system it may become apparent that the performance goal need not be precisely that originally specified or, alternatively, that the performance originally specified can be attained only at a cost much greater than originally proposed. The ordinary response to that circumstance in the 1960s was to adhere to original performance requirements or to incorporate requirements for improved performance and to accept the cost consequences of either action. In some cases the effort to incorporate performance substantially beyond the state of the art of the time has caused a great deal of money to be expended without ultimately providing the desired performance. The maximum speed of the F-106, the range of the B-58, and the supersonic range capability of the F-111 are relatively recent instances of originally specified performance that could not be attained at an acceptable cost. The structural weight requirement for the C-5A represents performance that was attained but probably was not worth the additional cost. When the discovery of probable performance shortfalls or recognition of the need for additional performance features occurs at an early stage of development a fresh examination of other alternatives is appropriate. For example, if some aspect of the originally required performance (or a more demanding performance requirement) is attainable only with the expenditure of a great deal of additional money, it might be highly desirable to choose among the available alternatives rather than simply to accept the costs of meeting the original or enlarged performance objectives.

Similarly, should a postulated threat change character before production commitments have been made, it would be reasonable to change an aircraft or missile system specification to compensate for the conditions of the changed threat. Such a change is less feasible from a design standpoint -- and more expensive -- if production has begun or if all the costly arrangements for production have been made. Again, should difficulty occur in development, it might be advisable to stretch

out the development schedule and incur the modest additional cost of resolving that difficulty instead of simply allocating vastly greater resources to the program (such as those involved in extensive retooling) in an attempt to resolve technical difficulty while maintaining the original schedule.

Finally, let us consider two premises often accepted by development agencies throughout the DoD: first, that the development and production aspects of acquisition must overlap, and second, that competition for production aspects of the acquisition phase cannot be conducted independently of competition for the development phases.

Implicit in the assertion that development and production must overlap is the assumption that in an incremental, sequential process there is an unconscionably long wait between the start of a program and the delivery of an operationally ready system. Rand studies conducted several years ago suggest that the time required for the development of a system by incremental processes is no greater than the time required for the development of a similar system by "concurrent" processes.* If that is the case, and the evidence is no stronger for one approach than the other, the "normal" strategy for system acquisition in the 1970s should involve *a conscious decision to produce (or not to produce) only after the development is completed.* However, setting down such a procedure does not guarantee compliance -- the proposed strategy is by no means foolproof. If the first part of the "production phase" is used to incorporate additional "easy" development improvements, the result could be a quick return to current practices.

The second premise, concerning the assumed infeasibility of separating competition for production from competition in the development

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*See B. H. Klein, T. K. Glennan, Jr., and G. H. Shubert, *The Role of Prototypes in Development*, The Rand Corporation, RM-3467-1-PR, April 1971. We should point out that under either "concurrent" or "incremental" procedures, *essential* long-lead-time tooling will have to be ordered during the development phase. The incremental strategy, however, avoids the cost of full production tooling before the system has been fully developed.

phase, is not supported by history. Experience with aircraft built under license by industry groups functionally and geographically independent of the development group suggests that technology transfer is in many instances quite feasible -- if not always easy.* In particular, it may be recalled that quite recently such complex aircraft systems as the F-104G, F-104J, T-33, F-4, and a surprisingly wide variety of European aircraft** have been built under license in countries other than those in which they were developed. In the 1950s, the B-47 aircraft was produced by both Douglas and Lockheed, as well as by its original developer, Boeing. The B-57 aircraft built in this country by Martin was a modified version of the Canberra light bomber developed by the English Electric Corporation in the late 1940s. Ships, aircraft, missiles, torpedos, tanks, and a large variety of avionics equipment have been similarly produced in plants quite distant and separate from those of the original developer. The same is true of a great many complex articles sold commercially. Although technology transfer has not been uniformly successful, there are few striking examples of extreme failures when production has been separated from development; conversely, several such programs have had very successful outcomes. Technology transfer makes separation of development and production feasible. If the sponsoring agency has all rights to the designs and data generated by a development program, competition in the production phase becomes feasible. The advisability of separating development from production depends on circumstances; its appropriateness would have to be determined on a program-by-program basis. But to disregard the option seems singularly shortsighted.

Austere Initial Development

Conducting an acquisition program in discrete phases with pauses deliberately and systematically introduced between the phases would

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*See also G. R. Hall and R. E. Johnson, *Aircraft Co-Production and Procurement Strategy*, The Rand Corporation, R-450-PR, May 1967.

**Including, among many, the Gnat, Magister, Mirage IIIS, Fiat G-90, and MiG-21.

encourage the sort of reassessment necessary to the tradeoff process sketched above. To be of maximum effectiveness, such a policy must include an additional objective -- program resources (particularly manpower) must be constrained. Large programs employing many thousands of people resist change and are difficult and costly to slow or redirect, however desirable the change or redirection may be.

It is particularly important that program resources be constrained early in the development phase, because it is there that most of the important changes resulting from "discoveries" are made. The notion introduced earlier, separating the development phase aimed at proving performance from subsequent phases aimed at making the system reliable and operationally suitable, suggests that resources should be sharply constrained during the "performance demonstration" phase. After the system design has demonstrated satisfactory performance, and the need for a system of that type has been reverified, it should be possible to proceed to the subsequent phases with considerable confidence that most of the major changes (and surprises) have already appeared. During development, information is the desired product, and one should attempt to purchase only information relevant to the problem being addressed if the goal is development efficiency.

Another way of stating the basic principle is that there are significant cost advantages to delaying work not relevant to the phase of development in progress. For example, it is unlikely in the extreme that spares consumption estimates made before the completion of an aircraft design will have much relevance to the requirements of that aircraft when it is introduced into service four to eight years later. On the other hand, spares and consumption rates and maintenance requirements could be calculated with fair accuracy once test articles were in hand for evaluation and system test time had been accumulated. Plainly, the designers of the initial system should pay attention to reliability and maintenance considerations; such considerations play a vital role in the initial design phase. However, the extensive and expensive work necessary to verify system reliability and to design a maintenance and spare parts stockage policy can and should be deferred until it is clear what the

system configuration will be and that the system will in fact be put into production and operation.

The feasibility of conducting successful development programs under resource constraints of this sort has been amply demonstrated in the past, and a few notable examples were mentioned in Section III. There are two classes of benefits that might accrue. One, noted above, is that the direction and goals of small, austere programs can be changed quickly and cheaply in response to technical problems or shifting requirements. There are numerous examples in the recent past of large development programs wherein major difficulties and deficiencies were uncovered quite early, but with a staff of several thousand people working on the project it was virtually impossible to delay or redirect the program to accommodate those problems. Another potential benefit of initial austerity is that it makes possible the funding of multiple, competitive sources during the early development phases. For example, the total development cost of a modern, high-performance fighter airplane today is in the neighborhood of $2 billion. The "preliminary development cost" of a conventional program, through flight test, approximates $250 million. However, a "performance demonstrator" could be designed, built, and flight tested for less than $75 million.* Hence, for an additional cost of well under 5 percent of the total development cost, it seems possible to carry a second, competitive development through the initial hardware phase.

Discussions of a low-cost performance-demonstration phase frequently elicit visions of an "experimental" system, something "built on hobby-shop tooling" that has little direct relation to the item subsequently put into quantity production. That is definitely not the approach being suggested here. Experience here and abroad demonstrates that by deferring expenses related only to quantity production or operational support (rate tooling, spares provisioning, extensive reliability testing, and

*Such an estimate is predicated not solely on recent European experience, but also on informal proposals from major U.S. constructors, unpublished calculations made at Rand several years ago, and the outcomes of a few contractor-funded programs of the 1960s.

so on) it is possible to build initial test items that are in major respects identical to the version then postulated for production. For example, in the Mirage and Viggen aircraft programs discussed in Section III, the initial development test items were structurally and configurationally identical to the intended operational versions; indeed, the tooling used in the fabrication of the initial items was subsequently used on the production aircraft.*

EXPECTED CONSEQUENCES AND BENEFITS

The potential advantages of the strategy proposed by this study are substantial. First, an incremental acquisition strategy in combination with a policy of austerity of resource expenditure during the early phases of development would definitely permit more new program starts at a cost equal to or even less than that incurred in the acquisition process at the present time. Indeed, even though it does not seem entirely possible to apply to American programs all of the practices favored abroad (for example, by the French), there are clear indications that major aircraft developments could be carried through the initial flight-test stage for amounts totaling no more than one-half and perhaps as little as one-fourth of the amounts now required to carry programs of a similar nature to that stage. If one accepts such a premise, it would be entirely feasible to carry two modestly different fighter aircraft through the initial development phase, and at the end of that phase to consider not merely which was the better of the two from a cost and performance standpoint, but which was better suited to the threat *then* anticipated. Nor is it inconceivable that the government could adopt a policy of purchasing all rights to products offered in such a competition and thus could integrate the better elements of two slightly different systems into one superior system through a further development phase. There would also be the option to make the production selection competitively and thus carry the buyer's bargaining leverage much further into programs than is possible under

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*See the more complete discussion of this point in Section III, p. 33.

present circumstances. The feasibility of technology transfer, which could allow competing for production as well as for design and development, has been previously discussed. The complications and possible shortcomings of such an approach clearly require a more thorough examination, but the evidence now available does not warrant a negative prejudgment of the feasibility of such an option.

Another obvious advantage of an incremental approach is that the cost of production articles could be more accurately estimated once an engineering test article reasonably representative of the desired final product becomes available. The feasibility of introducing specification changes at that stage should also be much clearer. And of course, introducing specification changes at the time production begins is appreciably less expensive than introducing such changes into ongoing production programs. Indeed, it is only prudent to consider very carefully the advantages and disadvantages of introducing production-phase specification changes as block changes, and deliberately avoiding the notoriously expensive process of retrofitting.*

Finally, because the predictability of a program outcome is improved by application of an incremental acquisition strategy, because costs for the development phase tend to be lower and more controllable, and because there is a higher probability that the system will be responsive to a threat that exists at the time the system is delivered, the credibility of the service proposing production of that system and postulating delivery costs should be appreciably better than is now the case. One of the objections to the current system of acquiring major weapons is that program outcomes are highly unpredictable. A demonstration, through one or two successful programs, that the services actually can anticipate program outcomes with reasonable precision should do a great deal to reestablish the credibility of the individual services. And that would be no small achievement.

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*One American contractor who has quite a lot of experience with retrofitting and with the introduction of significant changes in the production line estimated that the former process is about four times as expensive as the latter.

Some of the more commonly stated objections to an incremental acquisition strategy deserve consideration. For example, an incremental program, with deliberate pauses to evaluate the course of the program and its future prospects, might allow greater opportunity for program cancellation. For practical purposes, this is not a relevant objection. "Bad" systems have on occasion been hurried through development; they frequently are unsatisfactory in service use and tend to be phased out earlier than had originally been planned. Nor is development by a concurrent, compressed process any guarantee of program completion. In a development situation that encourages the start of several low-cost systems, the opportunity for carrying one of those through to operational delivery is appreciably higher than is the probability of completing a single, not very promising program.

Alternatively, it is often assumed that an incremental strategy incorporating the elements of sequential development will introduce unnecessary steps in the acquisition process. But whatever steps are necessary to the eventual development of an operationally useful system must be taken in any system development process. To approach them sequentially and to resolve one difficulty before taking on a second that arises from the first is not an exotic technique.

The proposition that making explicit cost-versus-performance compromises would cause the eventual delivery of systems that do not satisfy the *original* specifications does not seem relevant either. First, as suggested by the examination of experience of the 1960s, systems are being delivered today that in many respects do not satisfy original performance requirements. Second, as noted above, in many cases desired performance is never achieved even if a great deal of money is spent in seeking it. Third, in some cases the performance that is specified for a system is not always essential to the eventual operational use of the system. The threat may no longer be relevant, the operating conditions that originally suggested development of the system may have changed, or (as is frequently the case) a system obtained nominally for one military mission is used in quite another assignment.

An admission of uncertainty at the start of a program -- and the adoption of a program strategy requiring periodic reevaluation of program objectives, costs, and schedules -- will not necessarily suggest to DoD or Congressional authorities that such programs may not be carried to a successful conclusion. At present, it is difficult to sell almost any program to a DoD and a Congress that have little confidence in the program-outcome predictions made by the services. The product of an incremental acquisition strategy would in many instances be the resolution of uncertainties at an early stage in the acquisition process and at relatively low cost; thereafter, the services should be in a reasonable position to make a program projection that would be credible to the DoD and Congress.

IMPLICIT ORGANIZATIONAL RESTRUCTURING

The complexity of individual weapon systems has been steadily increasing. Partly in consequence, the cost of development and the unit cost of production items have increased greatly in recent years. Owing in part to the growing cost of major systems and in part to budgetary pressures that reduce the amount of funds available for the development and purchase of new systems, quantities purchased for the inventory have steadily decreased. Furthermore, the high and poorly predictable cost of developing and acquiring systems and the resultant decreased credibility of the advocating services have made it increasingly difficult for the services to obtain approval to begin new system developments. As a result, in the last decade new system starts have been less frequent, and planners therefore have felt obliged to attempt larger technological advances for new systems that enter development. A secondary but perhaps inescapable consequence of those circumstances has been the increasing difficulty of carrying systems through development successfully and expeditiously; both operating commands and developing agencies have found new systems troublesome during the period of their introduction into regular service.

It is clear that the adoption of an incremental acquisition strategy and a policy of austerity for the initial stages of development would

cause some *major* changes in existing institutions and policies, but such an approach has the potential of disrupting the unhappy cycle of events, noted above, that characterized the previous decade. Austere development, for example, requires smaller design groups, smaller development staffs, and thus obviously less expensive test phases than are customary at the present time. The size of the average industry group concerned with those phases of development would in all probability decrease very substantially. Additionally, were a policy of limited reporting and direct decision routes adopted, the size of the government institutions now concerned with development should decrease proportionately. Given the institutional pressures that exist, it may not be possible to reduce the size of government-monitoring establishments to those ordinarily encountered in West European countries or in such "exempt" programs as the Agena D. Nevertheless, project groups and decision-monitoring groups would, in all probability, employ fewer than one-half and perhaps as little as one-third as many people as are employed by the establishments now charged with acquiring major systems.

Securing an early commitment from senior officials of the DoD and from Congressional authorities is essential for a major program start. A policy of refusing to make early commitments to major programs and delaying decisions on production and deployment until there is substantial evidence that the system can be built for the assumed cost and can actually respond to a relevant threat would represent a significant departure from past policies. Changes that tend to reduce the size and therefore the cost of the present development establishment may well be inescapable, given current national budget trends. From the standpoint of the services, there is a good deal to be said for choosing a new acquisition strategy rather than having it imposed by the upper echelons of DoD or by Congressional authorities impatient with refusal of the services to respond appropriately to changed budgetary circumstances.

SUMMARY

The adoption of a strategy of incremental acquisition and a policy of austere development seems almost certain to result in lessened cost

growth and lower real cost. The predictability of schedule and performance outcomes seems almost equally certain to improve. That more system options could be generated at no increase in present system acquisition cost and that the options would be more responsive to emerging threats seem evident. Finally, systems would probably be delivered with fewer performance shortcomings than in recent years.

The evidence is not all in, nor does it make an absolutely clear case for the adoption of an incremental acquisition strategy. Further, an incremental acquisition strategy would not necessarily be equally and uncritically applicable to all of the systems proposed for development in the next decade. Quite obviously, threats may arise that will demand exemptions from the proposed strategy and acceptance of the risk of uncertain program outcomes and of proceeding toward major systems by processes approximating "concurrency." But even a cursory examination of experience in the last six or eight years suggests very strongly that few programs are driven to fruition by such a dominant threat.

For many if not most new systems, the attractions of an incremental acquisition strategy and austere development seem persuasive. Such evidence as is available supports the conclusions that a major change in the basic strategy of acquiring weapon systems is desirable and that austere development and an incremental acquisition strategy are very attractive options.

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